The little town of Trapp, at the immediate foot of the Blue Ridge on the Loudoun County (east) side, and about one and one-third miles from the Weather Bureau station, is over 1000 feet lower, its elevation above sea level being less than 700 feet. On the west side of the mountain the fall is less abrupt. According to the U. S. Geological Survey contour map the distance from the Mount Weather station to the nearest point on the Shenandoah River is about three miles, and the elevation of the river is between 300 and 400 feet. More exact determinations in the Shenandoah Valley will no doubt be made later.

THE PROPOSED COMPETITION IN FORECASTING AT LIEGE.

[Translation.]

UNIVERSITY OF CLERMONT,
METEOROLOGICAL OBSERVATORY OF PUY DE DOME,
CLERMONT-FERRAND, *January 27, 1905.*

Prof. WILLIS L. MOORE,
Chief U. S. Weather Bureau, Washington, D. C.

SIR: You have been pleased to communicate to me the letter written by you on January 7, last, to Mr. Jacobs, president of the Belgian Astronomical Society[1], in reply to the letter in which he invited you to become a member of an international jury charged with judging in a competition in weather forecasting which the Belgian Astronomical Society proposes to organize.

In accordance with your desire, I hasten to give on the subject the views which you do me the honor to request.

First of all, I had nothing to do with the editing of the document, or rather the proposed document, which was sent to us, and in forwarding my acceptance to Mr. Jacobs I made some express reservations and indicated especially that, in my opinion, the jury, when definitely constituted, should alone be qualified to decide upon the programme. I even made my acceptance conditional upon that of Mr. Teisserenc de Bort; convinced as I was in advance that if that eminent scientist consented to make one of the jury, his influence would be sufficient to have erased from the proposed programme whatever might be unscientific and give rise to well founded opposition.

I had not been consulted either as to my possible participation in the jury, and I should not have failed to protest—as you have done—if they had given my name as a member of the jury in a printed document destined to be given to the public; but I understood that it was only a proposed programme, and that in making use of my name in a printed proof I was left perfectly free to accept or to decline the invitation, and it was the same with all the others whose names appeared with mine.

Having given these preliminary explanations, it is very easy for me to tell you how heartily I am in accord with you as to the injury that is done to science by these fantastic prophets who, without any knowledge of the general movements of the atmosphere, forecast the coming weather somewhat after the manner of those who tell fortunes with cards, and whose blunders do not succeed in exhausting the credulity of the the public. It is necessary at any cost to prevent these from taking any part in a serious competition; and it was, in my opinion, very unfortunate that to the provision for a competition in forecasting for a proximate period they should have added a provision for forecasts several weeks in advance. It is evident that in the present state of science no such prediction can be made scientifically. My intention was to ask, in conjunction with Mr. Teisserenc de Bort, with whose ideas on these subjects I am well acquainted, the absolute elimination of this part of the programme, or rather this "side issue" added to the programme. I thought, however, that this side of the question could be more advantageously discussed when the jury had been constituted.

Again, I entirely agree with your view and those of Mr. Pernter when you say that it would be impossible to accept results, even if they should be excellent in themselves and verified by experience later, without knowing the methods by which they have been obtained; and I am firmly convinced that no prize should be adjudged to a metorologist for forecasts for very short periods in advance, unless he explains the details of his methods in such a way that afterwards any one else may be able to make use of it just as well as he.

The point upon which I take the liberty of differing with you, however, is in regard to the utility of a practical test by the author himself of a method of short-range forecasting. This question was discussed at the thirty-second meeting of the French Association for the Advancement of Science, held at Angers in 1903; the seventh section (Meteorology and Physics of the Globe), of which I had the honor to be president, formulated the following resolution:

"The seventh section, impressed by apparently proper methods for increasing the accuracy of weather predictions for short periods in advance, expresses the wish that the administration may give to the authors every facility for applying their methods under the most favorable conditions, and by appropriate tests, such as a competition, should allow competent scientists to pronounce as to the efficacy of these methods."

This resolution was adopted unanimously.

I can not but think that, in the present state of science, the prediction for the immediate future of depressions and centers of high pressure over Europe might be made with more precision than is ordinarily the case. Without entering into personal details, I may say that at this Congress of Angers the Section of Meteorology of the French Association was deeply impressed with the accuracy of certain forecasts applied to past conditions, and the French Association for the Advancement of Science, without itself taking the initiative for a competition, was won over to the idea that if those who think they can improve the methods of forecasting were put to the test and forced to apply their methods to a real prediction it would furnish the means of distinguishing that which is real progress from that which is only a repetition of what has been already done.

We do not lack persons who have general and very rational rules for predictions—to which indeed no objection can be made—but who, when charged with applying these rules, do not succeed in producing anything more than indications that are too vague to be of any real use. If those who think they can do better agree to submit to a severe test, and to explain afterwards their method of procedure, so that, by following them, others can derive profit from it, I can see in this only an excellent opportunity to separate what is serious and worthy to be called scientific from what is not. We must only take precautions. It will be especially necessary to abandon all idea of long-range forecasts, and carefully avoid anything that can furnish grounds for the criticisms—often so well founded—formulated by yourself and Mr. Pernter; but I think that the competition in itself, particularly if scientists of the standing of Mr. Teisserenc de Bort watch over it and exercise a control over its acts, would give rise to an exchange of ideas and discussions that would conduce to progress.

Believe me, dear sir, that this difference of opinion as to the utility of a *competition for forecasts for very short periods* does not prevent me from recognizing the correctness of your remarks, and I beg you to accept the assurance of my highest regard.

(Signed) BERNARD BRUNHES,
Director of the Observatory.

[1] *See* Monthly Weather Review, November, 1904, p. 523.

SOLAR HALO OF FEBRUARY 3, 1905, AT WASHINGTON, D. C.

By ERIC REX MILLER, Weather Bureau.

A solar halo observed at Washington, D. C., on February 3, 1905, deserves mention on account of its permanence and brilliant coloration; and especially because it was accompanied